Reviewer 2

We warmly thank the reviewer for the time and attention devoted to our paper, and for those positive and constructive comments. We have carefully considered all his comments and suggestions in the revised version of our manuscript. In what follows, our answers and modifications are highlighted in blue. Line numbers refer to the highlighted version of the revised manuscript.

This article describes the authors' analysis of an ensemble of simulations to analyze the SST variability in the Gulf of Thailand off the coast of Vietnam. They use a ten-member ensemble computed with a high-resolution ocean model to compare the impacts of eddies, tides, and river runoff during a two-year period. They find that the SST, which they characterize in terms of an "upwelling index", is mostly determined by winds, but in the Mekong Delta region they find that tides alter the currents and vertical turbulent heat transport enough to significantly influence the SST. This paper should be of interest to oceanographers and fisheries scientists interested in the detailed analysis of this region.

Overall, I found the paper well-written and logically laid out. My main questions and suggestions for improvement are as follows:

- I found the initial characterization of "upwelling" in terms of SST confusing. I think readers will be confused because upwelling normally refers to vertical velocity, but it is clear that it is really SST which is the focus of this article. I understand that this may be done because they want to use language and diagnostics consistent with previous analyses of this area, but it would be helpful to be explicit about this distinction.

This paper indeed belongs to an ensemble of studies, from the same authors and from other colleagues, that investigated the functioning and variability of upwelling that develops off the Vietnamese coast, in particular through its signature of SST (*Xie et al. 2003, 2007*, *Da et al. 2019, Ngo and Hsin 2021, To-Duy et al. 2022*, Herrmann et al. 2023 and many others cited in the paper). Our paper specifically aims at understanding the intraseasonal variability of the South Vietnam upwelling over its four areas of development, investigating in particular the effects of tides and rivers, and exploring the mechanisms involved in MKU functioning, that was revealed by *To-Duy et al. (2022*). We indeed wanted to be consistent with the language and diagnostics of those previous studies, hence using SST as an indicator of upwelling intensity. As explained by *Da et al. (2014*), upwelling indicators can be built based on surface wind (Ekman transport theory) or SST (see *Benazzouz et al., 2014*, for a review). The advantages of an SST-based indicators are, first, that they provide information on the upwelling intensity but also on the spatial distribution of upwelled water that reaches the surface and triggers primary production and, second, that it can be applied on real SST data derived from satellite observations to monitor the upwelling from observational data. To better understand the mechanisms involved in the upwelling functioning over the Mekong shelf, that was not studied until now, we also characterize the upwelling in terms of vertical velocity in part *4. Circulation mechanisms* of the paper).

→ Following this comment, we explained more clearly that we base our study of the upwelling on SST indicators (Introduction, page 4, lines 95-96 and Section 2.3, page 5, lines 150-154)

- (2) Because there is no discussion of degrees of freedom, I don't think that the t-test and F-test estimates of statistical significance are justified or useful. The discussion also mentions correlations between the FULL NoTides and NoRivers simulations, and I think this is a perfectly acceptable qualitative characterization of the results instead.

To quantify the effect of tides and rivers on the upwelling intensity and intrinsic variability, we indeed compute the relative differences $\Delta_m(UI_d)$ and $\Delta_\sigma(UI_d)$ of the mean (that quantifies the intensity) and standard deviation (that quantify its intrinsic variability) of the 10-member vector of yearly upwelling index UI_{JJAS} between the FULL reference simulation and in the NoRiver or NoTide sensitivity simulation, as explained in section 2.3.3. However, their sole values, reported in Table 1, do not allow to objectively estimate if those differences, hence the effect of tides or rivers, are significant or not. This is why, following Da et al. (2019), we compute the p-values p_m and p_σ associated respectively with the t test (for the mean difference) and F test (for the standard deviation difference): p_m and p_σ provide the degree of significance of those differences. We apply those tests on the 10-member vectors of UI_{JJAS} and in the reference and sensitivity simulation and report them in Table 1. We also compute p_m and p_σ to assess the significance of difference of daily upwelling index UI_d in Figure 2 of the manuscript, highlighting in colors the periods when the differences are significant at more than 99% (p-value<0.01).

In the paper, we also investigate the relationship between the chronology over JJAS of several variables by computing the Pearson correlation coefficient and associated p-values (that again quantify the statistical significance of the correlation) between their 122-day time series.

→ We explained that more clearly in the revised manuscript, writing in particular that t and F tests are performed over 10-member size vectors, and that correlations are computed between 122-day size time series, which therefore inform about the degree of freedom (Section 2.3.3, page 7, lines 192-211). We moreover simplified the explanations of OIV indicator in part 2.3.2, replacing the generic "X" variable by the UI_d and UI_{JJAS} variables over which we actually perform the computation at the daily and summer (JJAS) scales.

- (3) Because the results find, essentially, that the tides influence Mekong upwelling, and the other regions are not much affected, I think the other regions could mostly be left out of the discussion. Perhaps the discussion of the other regions could be emphasized only in the introductory material. Similarly, in the abstract, consider re-ordering the presentation so that the positive or significant results are stated first, and the no-impact results are stated as a contrasting follow-up.

Since our study focuses in the SVU, since previous studies questioned the influence of tides and rivers on SVU areas other than MKU, and given the questions of the other reviewers, we chose to keep the results concerning the effect of tides and rivers on OFU, NCU and SCU. We however followed the suggestion of the reviewer by reorganizing the Abstract, Part 3 (*Impact of tides and rivers in the four upwelling areas*) and Conclusion, reducing as much as possible the non significant results, and presenting the significant results first (see Abstract, page 1, lines 11-19 ; Section 3, pages 8-10, lines 221-300 and Conclusion, pages 16-17, lines 506-516).

- (4) I think the article would be more impactful if it emphasized the analysis of the term balance of the temperature evolution equation. You could map the Simpson-Hunter number (the ratio of surface heat flux to tidally-generated vertical turbulent heat flux) to delineate the regions where you would might expect tidal currents to be significant in the SST budget. Since you mention the significance of locally-created vs non-local (horizontally-advected) stratification, could you quantify this by mapping an appropriate non-dimensional number. Likewise, in the analysis of divergence you highlight the role of topographic features. This could be captured by comparing bottom u*grad(H) vs. H*div(u) at mid-depth.

- To highlight the significance of locally-created vs non-local (i.e. horizontally-advected) surface cooling, we examine the maximum over the water column of the difference of $log_{10}(Kz)$ between FULL and NoTide, $\Delta log_{10}(Kz)$, where Kz is the vertical diffusivity coefficient. $\Delta log_{10}(Kz)$ quantifies the vertical mixing induced by tides: a value higher than 2 means that Kz is 10^2 higher with tides than without tides, i.e. that the vertical mixing induced locally by tides dominates the vertical mixing (see profiles of temperature and Kz in Figure 9 of the paper). Conversely, a value lower than 1 means that tides do not significantly contribute to the local vertical mixing. We show $\Delta log_{10}(Kz)$ on figure Ad below, together with the maps of SST in FULL and NoTide and of their difference. We also indicate the -0.4°C contours of the SST difference between FULL and NoTide : it highlights the area of surface cooling induced by tides. High values of *$\Delta log_{10}(Kz)$* in this area *(>2)* corresponds to tidally induced surface cooling due to local tidal vertical mixing (e.g. points F, E, D, as shown in Fig. 9 of the paper). Low values (<1) correspond to tidally induced surface cooling due to horizontal advection of water mixed upstream (e.g. point A downstream point F).

→ we added this in the revised version of the paper (Section 4.2.2, pages 14-15, lines 440-4476 and, Figure 9)

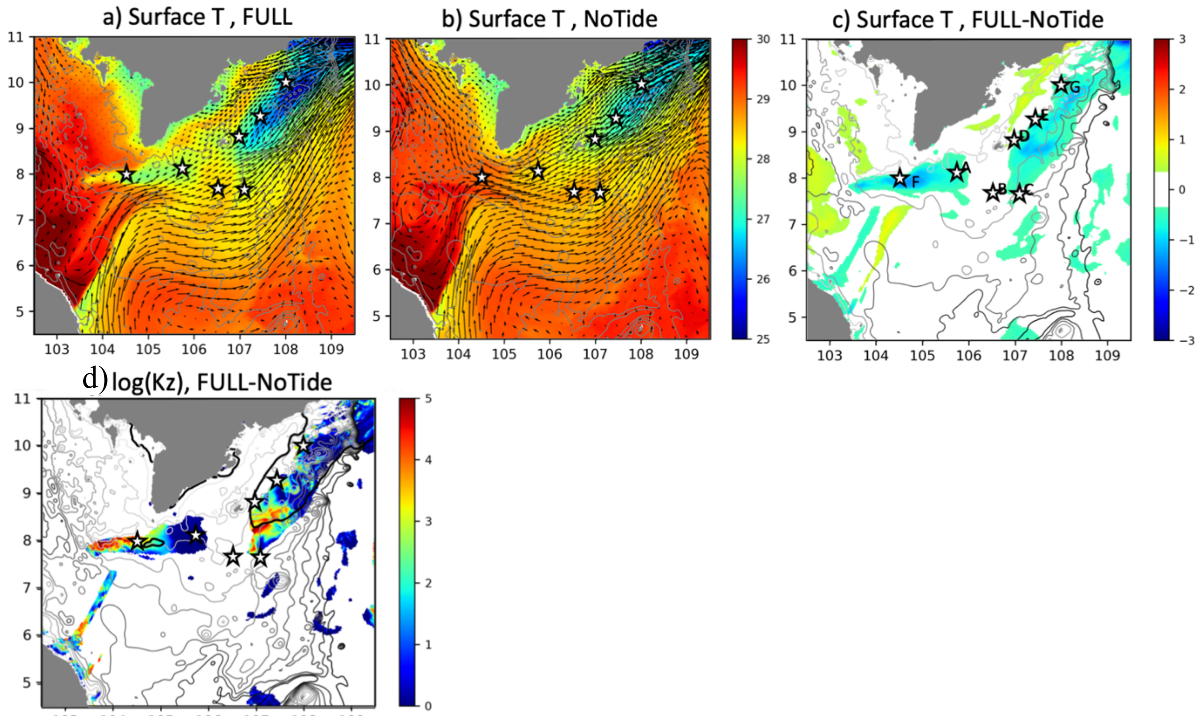

Figure A : Maps of SST on 16/07/2018 in M17 of the FULL ENSEMBLE (a), of the NoTide ensemble (b) and their difference (c), and map of $\Delta log_{10}(Kz)$ on the same day plotted only for areas where the SST difference exceeds 0.4°C. The black line shows the isotherm of To=27.6°C that corresponds to the region of upwelling occurrence.

- To highlight the effect of topography, we plot below in Figure B the vertical velocity induced by the topography gradient, u_{bottom} . grad(h) (Fig. Ba), and the ratio between u_{bottom} . grad(h) and the surface velocity (Fig. Bb), following the suggestion of the reviewer. We can see spots of strong values of topography induced vertical velocity over places of steep topography, in particular some small sea mounts (see Fig. Ca that shows the bathymetry), suggesting and effect of topography. This effect is however not really highlighted by the ratio figure. In order to highlight the role of topography in a more convincing way, we therefore performed an additional simulation (with rivers and tides) where we smoothed those topographic anomalies, and compare the resulting vertical velocity with the FULL ensemble but also the LONG simulation performed with the same model configuration over the period 2009-2018 by *To-Duy et al. (2022).*

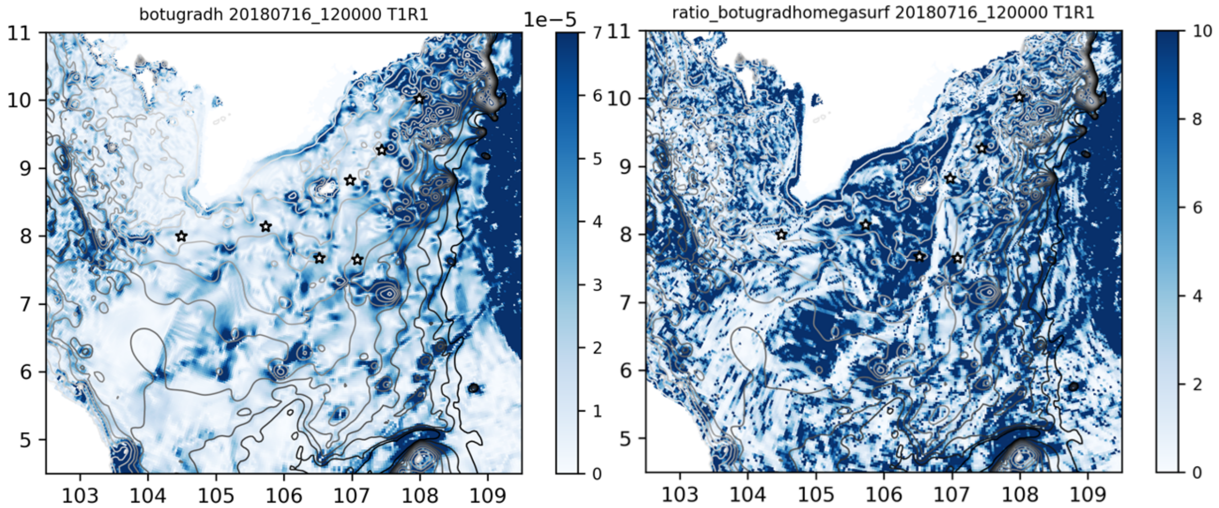

Figure B : maps of bottom vertical velocity induced by topography gradient (u_{bottom}.grad(h), m.s^{-1}, left) and ratio between u_{bottom}.grad(h) and the surface vertical velocity (right).

We show in Fig. Ce-h below the surface vertical velocity on July 16th, 2018 for 2 members of the FULL ensembles (the other members, not show, are extremely similar), for the LONG simulation and for the simulation with smoothed bathymetry. The 10 members of the FULL ensemble, but also the LONG simulation of *To-Duy et al. (2022*) show extremely similar positions and values of strong surface velocity (see dashed line on Fig. Cd-e). In the simulation with the smoothed bathymetry, the positions of strong vertical surface velocity change, with the line of strong positive vertical velocity near the dashed line shifted by ~30 km to the west. Modifying the topography, removing in particular the small seamounts, therefore modifies in this sensitivity test the upwelling location that hardly changes varies within the FULL ensemble and in the LONG simulation, confirming the determining role of topography in this location.

→ We added this in the revised version of the paper (Section 4.1, page 12, lines 352-360, and Figure 7).

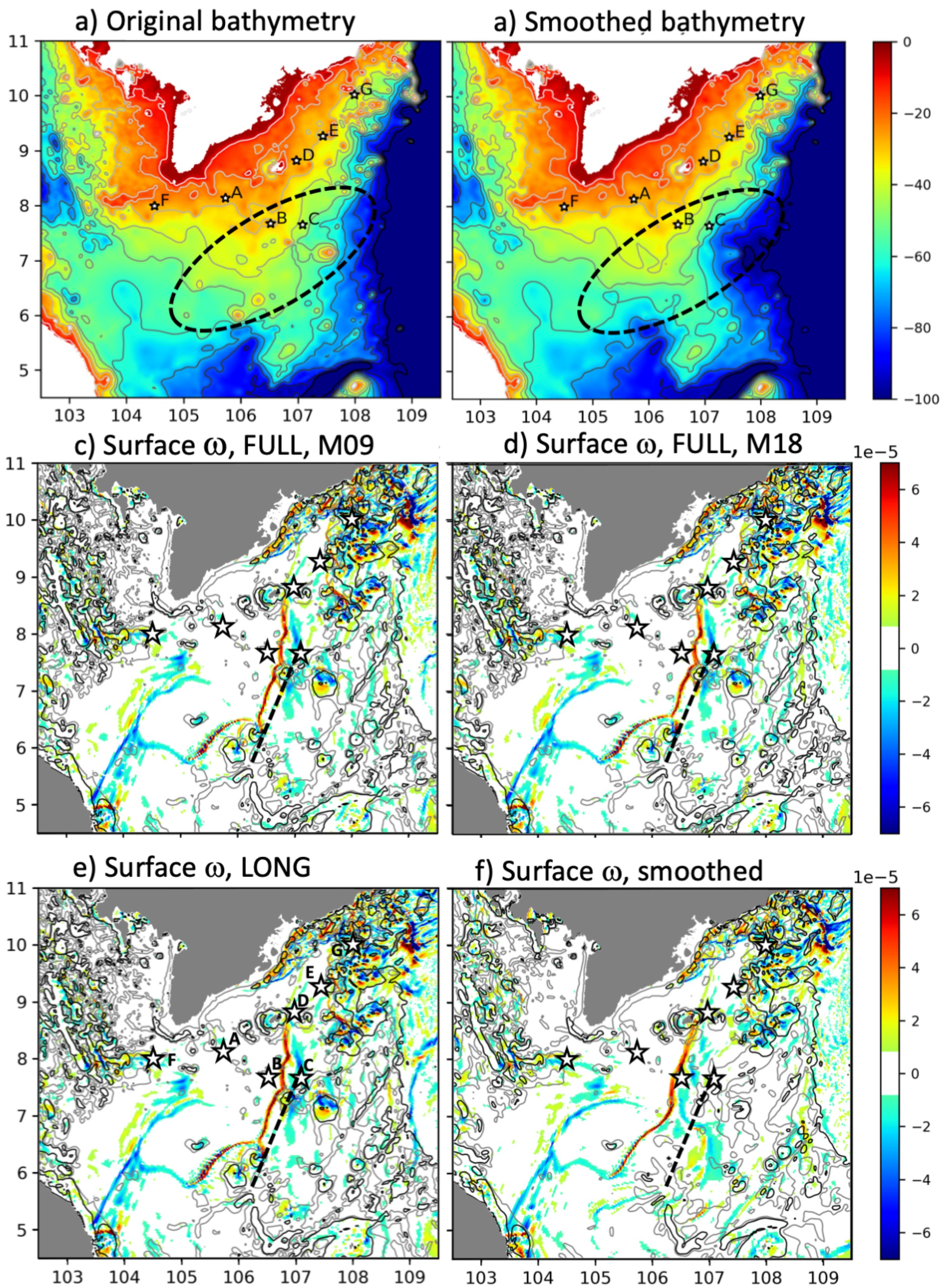

Figure C: Initial bathymetry (a, m) and smoothed bathymetry (b), the dashed ellipse shows the area of bathymetry smoothing. Surface vertical velocity (m.s^{-1}) on 16/07/2018 in M09 (c) and M18 (d) of the FULL ensemble (the other members, not shown, are extremely similar), in LONG (e) and in the sensitivity simulation with smoothed bathymetry (f). The dashed line highlights the front of strong upward vertical velocity.

- Investigating into details the heat budget over the area, quantifying the role of the relative contributions of atmospheric fluxes, lateral transport and vertical advection and mixing is indeed a question that we plan to address. We show in Fig. A the daily timeseries atmosphere net heat flux over the region: a few periods of negative heat fluxes, i.e. inducing ocean surface cooling, can be observed during some wind peak events. Those periods are however very short with a heat loss hardly exceeding 50 $W.m^{-2}$. Moreover, though the NoTide simulation is submitted to a very similar atmospheric heat flux, it produces a much weaker upwelling as explained in details in the paper. This suggests that the effect of atmospheric cooling is much weaker than the effect of vertical velocity and of vertical tidal mixing. We could answer in details to this question using the online closed water, heat and seat budget tool available in the mode, as we did in the study of Trinh et al. (2024) with a 4 km resolution configuration of SYMPHONIE over the South China Sea to examine the relative contributions of lateral fluxes at interocean straits, river and atmospheric forcing and internal variations in the seasonal variability of those budgets. Beside, the effect of the upwelling and associated surface cooling to the atmosphere has also been mentioned in the literature, in particular its effect on sea breeze winds (*Zheng et al. 2016, Yu et al. 2020*). We plan to investigate this question with the ocean-atmosphere coupled SYMPHONIE-RegCM model recently developed in the framework of the *Quentin Desmet (2024)* PhD to study air-sea coupled interactions in the region. As explained above, the present paper really focuses on the upwelling functioning and variability, that we quantify and explain based on SST, but also on horizontal and vertical velocity. The study of heat (as well as water and salt) budget would not only require additional simulations, we moreover think that we would deserve a dedicated analysis and paper.

→We therefore choose not to investigate the question of air-sea heat fluxes here, but mention this question in the manuscript (Conclusion, page 17-18, lines 540-547).

- (5) There is no discussion to justify the randomization technique used to create the ensemble. The approach taken seems perfectly reasonable, but it would be good to discuss why the 100km cutoff between large-scale and small-scale is appropriate, and also to mention why other sources of randomness (such as the winds or large-scale stratification) were not used.

Indeed, in the submitted version of the manuscript, we only referred to *Herrmann et al. (2023)* where the ensemble creation strategy was explained. This strategy is based on the fact that most of the OIV develops at (sub)mesoscale, related in particular to the presence of eddies of strongly chaotic behavior. *Herrmann et al. (2023)* indeed explained: "*For that we used ten different initial conditions for temperature, salinity, sea surface elevation and currents fields. Most of the OIV develops at mesoscale (Sérazin et al., 2015, Waldman et al., 2018), we therefore only perturbed the mesoscale field, following the same methodology as Waldman et al. (2017a, 2017b, 2018). For the ten simulations of the ensemble, the large-scale state of the initial field is identical, and the small-scale of the initial field state differs. The common large-scale state is equal to the large-scale state of January 1st, 2017 of the LONG simulation, computed using a 100 km low-pass filter. For XX going from 09 to 18, the small-scale state of January 1st, 20XX of the LONG simulation is computed using a 100 km high-pass filter.*" The 100 km cutoff was therefore chosen to separate the large scale circulation from the (sub)mesoscale processes, which develops at scale smaller than 100 km (*Lin et al. 2020, Ni et al. 2021*).

Indeed, as pointed out by the reviewer, applying perturbation on the atmospheric fields that drive the upwelling (i.e. the wind, see for example *Nguyen-Duy et al. 2023*) or to the lateral boundary conditions that would create a different circulation fields at the submesoscale to mesoscale (see for example *Da et al. 2019*) would also introduce chaoticity in our simulations, and the study of their effect would be of interest. Perturbing the large scale stratification could also be a source of chaoticity, but would also represent factors other than the sole chaoticity effect, in particular the effect of interannual to long term atmospheric conditions and lateral oceanic forcing, making the sensitivity tests difficult to interpret as a result of chaoticity alone.

→ We added a more detailed explanation about the perturbation strategy in the revised version of the manuscript, mentioning also the role of other perturbing factors as winds and lateral boundary conditions (Section 2.2, page 5, lines 139-142 and Conclusion, page 18, lines 548-550).

Overall, I think this careful analysis will be of interest to researchers studying this region. I recommend publication after minor revision, if the authors choose; or they might wish to pursue the more extensive revision implied by item (4), above.

As explained above, we chose to examine the questions rose by the reviewer in (4), in particular regarding the relative local vs. remote effects of tides on sea surface cooling, and the role of topography.

[Dear authors and editors:

I spent the last 2.5 hours reading this manuscript and enumerating my detailed comments in this text box. Unfortunately, when I clicked "Intermediate save" the website asked me to authenticate again, and all of my comments were lost when this web page again re-opened. I am not willing to re-create my detailed comments. I have re-created my general comments, above.]

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